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*THE PLANT INDIVIDUAL IN THE LIGHT OF EVOLUTION.**

THE PHILOSOPHY OF BUD-VARIATION, AND ITS BEARING UPON WEISMANNISM.

I.

WHILST the animal and vegetable kingdoms originate at a common point and are not clearly distinguishable in a number of the lower groups or organic beings, they nevertheless diverge rapidly and they finally become very unlike. I believe that we shall find that this divergence into two coördinate branches of organic nature is brought about by the operation of at least two fundamentally distinct laws. There is a most unfortunate tendency, at the present time, to attempt to account for all phenomena of evolution upon some single hypothesis which the observer may think to be operative in the particular group of animals or plants which he may be studying. For myself, I cannot believe that all forms of life are the results of any one law. It is possible that all recent explanations of evolution contain more or less truth, and that one of them may have been the cause of certain developments, whilst others have been equally fundamentally important in other groups of organisms. If I were a zoölogist, and particularly an entomologist, I should hold strongly to the views of Lamarck; but, being a horticulturist, I must accept largely, for the objects which come within the range of my vision, the principles of Darwin. In other words, I believe that both Lamarckism and Darwinism are true; and, in this connection, it is significant to observe that Lamarck propounded his theory from studies of animals, whilst Darwin was first led to his theory from observations of plants. I am willing to admit, also, at least for the sake of argu-

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ment, that Weismannism, or the Neo-Darwinian philosophy, may be true for some organisms, but it is wholly untenable for plants.

There is one feature of this difference between the animal and the plant to which I wish to call your attention on this occasion. It is the meaning of individuality in the two. I must say, at the outset, that when I speak of a plant or an animal I refer to those higher forms which the layman knows by these names, for it is not my purpose to discuss the original causes of divergence so much as those phenomena of individuality which are most apparent to the general observer. The animal may be said to have complete autonomy. It has a more or less definite span of life. It grows old and dies without having been impaired by decay, and the period of death may have no immediate relation to environment. It has a definite number of parts, and each part or organ is differentiated and performs one function, and this function serves the whole animal and not the organ itself. If any part is removed the animal is maimed and the part cannot be supplied, and the severed portion has no power to reproduce either itself or the animal from which it came. The only means by which the animal can multiply is by a union of sexes. The plant, on the contrary, has no perfect or simple autonomy. It has no definite or pre-determined proximate span of life, except in those instances when it is annual or biennial, and here duration is an evident adaptation to environment. The plant frequently dies as the result of decay. It has not a definite number of parts, and each part of the plant may perform a function for itself, and the part may be useful to the remainder of the plant or it may not. One part is like what all other parts are or may be. If one portion is removed the plant may not be injured; in fact, the plant may be distinctly benefited. And the severed portion may not only have the power of reproducing itself, but it may even reproduce an organism like that from which it came. In other words, plants multiply both with and without sex. Potentially, every node and internode of the plant is an individual, for it possesses the power, when removed and properly cared for, of expanding into what we call a plant, and of perfecting flowers and seeds and of multiplying its kind.

Those of you who are botanists now recall the contention of Gaudichaud concerning the plant unit or phyton. He proposed that the leaf, with its connecting tissues, is the vegetable individual and that the plant is a colony of these individuals. Gaudichaud offered this theory as an explanation of the morphology and physiology of plants, and the hypothesis really has no place in the present discussion; but, inasmuch as I have borrowed the word which he proposed for the plant unit, it is no more than fair that I should explain his use of it; and this explanation may serve, incidentally, to illustrate some of the problems of individuality to which I shall recur. Gaudichaud, while recognizing that a cell which develops into a bud is itself an individual, nevertheless considered that the leaf, with its dependent tissues, represents the simple vegetable unit. Each of these units has an aerial or ascending part and a radicular part. The ascending part has three kinds of tissues or merithals—the stem merithal, petiolar merithal and the limbic merithal. Now, each phyton fixes itself upon the trunk or upon an inferior phyton, in the same manner as a plant fixes itself in the soil, and, sending its vascular threads downwards between the bark and the wood, is enabled to support itself upon the plant colony; and, at the same time, the extension of these threads produces the thickening of the stem, and the superposition of phytons increases the height of the plant. This mechanical theory of the morphology

of plants was not original with Gaudichaud, but he greatly enlarged it and gave it most of its historic value, and, what is more to our purpose, he used the word phyton, which, in lieu of a better one, I shall use as a convenient expression for that asexual portion of any plant which is capable of reproducing itself. Gaudichaud's fanciful hypothesis was not completely overthrown until the exact studies of Von Mohl upon the vegetable cell established a rational basis of morphology and physiology.

What I wish now to show is that the evolution of the vegetable kingdom cannot be properly understood until we come to feel that the phyton, or each portion of the plant, which, when removed, has the capability of reproducing itself and its parent, is in reality a potential autonomy. In doing this I shall not forget that the plant also has an individuality as a whole, but as this feature is quite aside from my argument and is the conception of the plant which is everywhere accepted, I shall necessarily confine my remarks to the individual life of the phyton. The mere fact that the phyton may reproduce itself is not the most important point, but, rather, that each part of the plant may respond in a different manner or degree to the effects of environment and heredity. Before proceeding to this matter, I should say that there is no doubt about the capability of every plant to be propagated asexually. It is true that all plants have not been so propagated, but there is every reason to suppose that the gardener can acquire the requisite skill to grow oaks and hickories from cuttings were it worth his while to do so. At present there are cheaper modes of multiplying these plants. But certain pines and spruces, which do not seed under cultivation, are propagated by cuttings, and the tissue of these trees is as little adapted to such use as that of any plants with which I am acquainted. The fact that plants are not grown from cuttings does not prove that they cannot be so propagated, for we know that the essential structure of all of them is very similar, and that each node and internode—or each phyton—does or may produce branches and flowers and seeds when it is borne upon its parent plant. And I should remind you that those plants which are not readily multiplied by cuttings are generally propagated by grafting, which, for illustration, amounts to the same thing, for we only substitute the stock of another plant for the soil. Plants of the most various kinds are readily multiplied by graftage. Even tuberous herbaceous stems, which are not commonly associated with the art of the grafter, unite with ease. One of the latest investigators in this field is a Frenchman, Daniel, and his conclusions upon the physiology of grafted plants show that the physiological modifications in these plants are largely such as arise from physical causes, showing that the parts still preserve their essential autonomy.

Now, if every plant varies in the number of parts, or phytons, of which it is composed, it follows that this number must be determined by agencies which act immediately upon the given plant itself. We all know that the number of these parts is determined very largely by environment. A dozen plants springing from the same capsule may vary immensely in the numbers of their branches, leaves and flowers, and this variation is generally obviously correlated with amount of food, amount of space which the plant is allowed to occupy, and other physical conditions which affect its welfare. But we not only find that no two plants have the same number of parts, but that no two branches in the same plant are alike. One part grows longer, one more erect, one has greener leaves, one bears more fruit. So, too, there may be different forms of flowers on the same plant, a subject to which Darwin has devoted an entire

volume. We know, also, that this variation amongst the sisterhood or colony of branches is determined by very much the same conditions which determine variation in independent plants growing in soil. I believe that the primary and most important determinant of this variation is the variation in food supply, the same which Darwin believed to be the most potent factor in the origination of variations in general. That branch or phyton which receives the most food, because of its position or other incidental circumstance, is the one which grows the largest, has the heaviest and greenest leaves, and, in the end, is the most fruitful. I use the word food to designate not only the supply of nutriment which is derived from the soil, but also that obtained from the air and which is most quickly and thoroughly elaborated in the presence of the brightest sunlight. Thus the uppermost branches of the tree, whilst farthest from the root, are generally the strongest, because they are more freely exposed to light and air and their course is least impeded. Many branches in the interior of tree tops are undoubtedly parasites upon the plant colony, taking from it more than they return.

If the number of the plant units is determined by circumstances peculiar to that plant, and if there is variation amongst these units in any plant, then it follows that there must be struggle for existence between them. And this struggle differs from the conflict between independent plants in the complex battle for life only in the circumstance that it is more intense or severe, from the fact that the combatants are more closely associated. There are weak branches and strong branches, and the survival of the fittest is nature's method of pruning. The strong terminal branch, shooting upwards towards air and sunlight, makes the bole of the tree, whilst the less fortunate or side branches perish and fall. The leaf surface of any tree or large plant is always pushing outwards towards the periphery, which is only another way of saying that the anterior branches die. I often find fruit growers who refuse to prune their trees because they believe it to be unnatural, while at the same time their tree tops are full of dead limbs, every one a monument to the stupidity of the owner!

Now, the effect of this struggle for existence allows of mathematical measurement. Each bud should produce a branch or a cluster of fruit. A seedling peach tree may be two feet high the first year, producing thirty leaves, and in every axil a bud. Each of these buds should produce a branch, which should again produce thirty buds. The third year, therefore, whilst the tree is only six or eight feet high, it should have 900 branches, and in the fourth year 27,000! Yet a peach tree twenty years old may not have more than 1,000 branches! That is, many millions of possible branches have been suppressed or have died. I once made an actual observation of such a battle and counted the dead and wounded. A black cherry tree came up near my door. The first year it made a straight shoot nineteen inches high which produced twenty-seven buds. It also sent out a branch eight inches long which bore twelve buds. The little tree had therefore enlisted thirty-nine soldiers for the coming conflict. The second year twenty of these buds did not grow. Nineteen of them made an effort, and these produced 370 buds. In two years it made an effort, therefore, at 409 branches, but at the close of the second year there were only twenty-seven branches upon the tree. At the close of the third year the little tree should have produced about 3,500 buds or branch-germs. It was next observed in July of its fourth year, when it stood just eight feet high; instead of having between 3,000 and 4,000 branches, it bore a total of 297, and most of them were only weak

spurs from one to three inches long. It was plain that not more than twenty, at the outside, of even this small number could long persist. The main stem or trunk bore forty-three branches, of which only eleven had much life in them, and even some of this number showed signs of weakness. In other words, in my little cherry tree, standing alone and having things all its own way, only one bud out of every 175 succeeded in making even a fair start towards a permanent branch. And this struggle must have proceeded with greater severity as the top became more complex, had I not put an end to its travail with the axe!

II.

I am now ready to say that I believe bud-variation to be one of the most significant and important phenomena of vegetable life, and that it is due to the same causes, operating in essentially the same way, which underlie all variation in the plant world. As some of you may not be familiar with the technical use of the term, I will explain that a bud-variety is an unusual or striking form or branch appearing upon a plant; or, as Darwin put it, bud-variation is a term used to "include all those sudden changes in structure or appearance which occasionally occur in full-grown plants in their flower-buds or leaf-buds." A classical example is the origination of the nectarine from a branch of a peach tree; and one often hears of Russet apples upon a certain branch of Greening apple tree, of weeping, variegated or cut-leaved shoots on otherwise normal trees, or of potatoes that 'mix in the hill.' Now, this matter of bud-variation has been a most puzzling one to all writers upon evolution who have touched upon it. It long seemed to me to be inexplicable, but I hope that you will now agree with me in saying that it is no more unintelligible than seminal variation of plants, for I have already shown that there is abundant asexual variation (of which bud-variation is itself the proof), and that this variation takes place as readily when the phyton is growing upon a plant as when it is growing in the soil. The chief trouble has been, in the consideration of this subject, that persons have observed and recorded only the most marked or striking variations, or those which appear somewhat suddenly (although suddenness of appearance usually means that the observer had not noticed it before), and that they had therefore thought bud-variation to be rare and exceptional. The truth is, as I have said, that every branch or phyton is a bud-variety, differing in greater or lesser degree from all other phytons on the same plant. These differences, even when marked, may arise in every part of the parent plant, as on stems aerial and subterranean, from bulbs and tubers, or even from the adventitious buds of roots; and the characters of these varieties are as various as those originating from seeds. The nurseryman knows that branches differ amongst themselves, for he instructs his budders to cut buds only from the top-most shoots of the nursery rows in order that he may grow straight, vigorous trees; and every farmer's boy knows that the reddest and earliest apples grow on the uppermost branches, and his father will always tell him that he should never select cions from the center or lower part of a tree. Every skilful horticulturist will tell you that the character of the orchard is determined very largely by the judgment of the propagator in selecting cions. To select out the extreme forms of these variations and to attempt to explain bud-variation by them is exactly like selecting the extreme types of seminal variations, and, by ignoring the lesser ones and the intermediates, to attempt to build thereon a theory of the variation of plants. If you ask me why it is that the nectarine was produced upon a branch of a peach

tree I will answer that nectarines have also been produced from peach seeds. The answer to one answers the other. It is true that bud-variations, if we use that term, as we logically must, to denote all variations between phytons, are commonly less marked than seed-variations, but this is only because the conditions of origin and environment of the phyton are less varied than those of the seedling. The phytons originate from one parent, not from two; and they all grow in very like conditions. But I am convinced that, when we consider the plant individual in the light of evolution, the bugbear of bud-variation vanishes.

A good proof that bud-variation and seed-variation are one in kind is afforded by the fact that selection can be practiced for the improvement of forms originating by either means. Darwin was surprised, as he says, to "hear from Mr. Salter that he brings the principle of selection to bear on variegated plants propagated by buds, and has thus greatly improved and fixed several varieties. He informs me that at first a branch often produces variegated leaves on one side alone, and that the leaves are marked only with an irregular edging, or with a few lines of white and yellow. To improve and fix such varieties he finds it necessary to encourage the buds at the bases of the most distinctly marked leaves and to propagate from them alone. By following, with perseverance, this plan during three or four successive seasons a distinct and fixed variety can generally be secured." This practice, or similar ones, is not only well known to gardeners, but we have seen that nature selects in the same manner, through the operation of the same struggle for subsistence which Darwin so forcibly applied to all other forms of modification. Once given the three fundamental principles in the phylogeny of the phyton, the variation amongst themselves, the struggle for existence, the capability of perpetuating themselves—an indisputable trinity—and there can no longer be any doubt as to the fundamental likeness of the bud-variety and the seed-variety.

Yet I must bring another proof of this likeness to your mind. It is well known that the seedlings of plants become more variable as the species is cultivated; and it is also true that bud-varieties are more frequent and more marked in cultivated plants. Note, for example, the tendency of cultivated plants to bear variegated or cut-leaved or weeping shoots, and the fact that the colors and doubleness of flowers often vary greatly upon the same plant. Many of our best known roses, carnations, chrysanthemums, violets and other garden plants originated as bud-sports. This fact is so well known that critical gardeners are always on the alert for such variations. In any house of 200 roses, all grown from cuttings, the grower will expect to find more than one departure from the type, either in color or freedom of bloom or in habit of plant. Every gardener will recall the 'sporting' tendencies of Perle des Jardins rose, and the fact that several commercial varieties have sprung from it by bud-variation. As early as 1865 Carrière gave a descriptive list of 154 named bud-varieties, and remarked at length upon their frequency amongst cultivated plants. This fact of greater bud-variability under cultivation was fully recognized by Darwin, and he regarded this as one of the strongest proofs that such variation, like seed-variation, is "the direct result of the conditions of life to which the plant has been exposed."

In order to extend the proofs of the essential ontogenetic likeness of bud and seminal variations, I will call to your remembrance the fact that the characters of the two phytons may be united quite as completely by means of asexual or graft hybridism as by sexual hybridism. I do not need to pursue this subject, except to say that we now believe that graft-hybrids are rare

and exceptional chiefly because the subject has received little experimental attention. Certainly the list given by Focke, and the anatomical researches of Macfarlane, show that such hybrids may be expected in a wide variety of subjects and with some frequency. It is now stated positively by Daniel, as the result of direct experiment, that the seeds of cions of certain cultivated herbs which are grafted upon a wild plant give offspring which show a marked return to the wild type. I should also add that the breaking up of seminal hybrids into the characters of either parent may take place, as Darwin has shown, through either seed- or bud-variation. You are all no doubt aware that hybrids generally tend to revert to the types from which they sprung, and this sometimes occurs even in hybrid offspring which is propagated exclusively by buds or cuttings.

Still another proof of the similarity of bud-varieties and seed-varieties is the fact that the seeds of bud-varieties are quite as likely to reproduce the variety as the seeds of seed-varieties are to reproduce their parents. Darwin and others have recorded this seminal transmission of bud-sports. "Notwithstanding the sudden production of bud-varieties," Darwin writes, "the characters thus acquired are sometimes capable of transmission by seminal reproduction. Mr. Rivers has found that moss-roses [which are bud-varieties] generally reproduce themselves by seed; and the mossy character has been transferred by crossing from one species to another." This general fact that bud-sports may reproduce many of their essential acquired characters by seeds is so well grounded in the minds of gardeners that the most critical of them make no distinction, in this respect, between varieties of bud and seed origin when selecting parents for making crosses. And if we can prove the similarity of bud and seed variations by showing that both bear the same relation to transmission of characters by means of seedage, we can demonstrate it equally well by the converse proposition—that both bear the same relation to the perpetuation of their features by cuttings. Some seed-varieties will not 'come true' by cuttings, and there are also some bud-sports which will not, as every gardener of experience knows. I will cite a single case of 'sporting' in bud offspring. One winter a chance tomato plant came up in one of my greenhouses. I let it grow, and it bore fruit quite unlike any other variety which I ever saw. There was no other tomato plant in the house. I propagated it both by seeds and cuttings. I had two generations of cuttings. Those taken directly from the parent plant, 'came true' or very nearly so; then a lot of cuttings from these cutting-grown plants was taken, making the second asexual generation from the original seedling. While most of the seeds 'came true,' few of these second cuttings did, and, moreover, they 'sported' into several very unlike forms—so much unlike that I had both red and yellow fruits from them. In respect to transmission of characters, then, bud- and seed-varieties are alike, because either class may or may not transmit its marks either by seeds or buds.

Finally, let me say, in proof of the further similarity of bud- and seed-variations, that each class follows the incidental laws of external resemblance which pertain to the other class. For instance, there are analogous variations in each, giving rise to the same kinds of variegation, the same anomalies of cut and colored foliage, of weeping branches, party-colored fruits and the like; and the number of similar variations may be as great for any ameliorated plant in the one class as in the other. The most expert observer is not able to distinguish between bud-varieties and seed-varieties; the only way of distinguishing the two is by means

of the records of their origins, and because such records of any varieties are few we have come to overlook the frequency of bud-variation and to ascribe all progressive variability in the vegetable kingdom to seeds or sex.

Whilst it is not my purpose to discuss the original sources of bud-variations, I cannot forbear to touch upon one very remarkable fact concerning reversions. It is a common notion that all bud-varieties are atavistic, but this position is untenable if one accepts the hypothesis, which I have here outlined, of the ontogenetic individuality of the phyton, and if he holds, at the same time, to the transforming influence of environment. It is also held by some that bud-varieties are the effects of previous crossing, but this is controverted by Darwin in the statement that characters sometimes appear in bud-varieties which do not pertain to any known living or extinct species; and the observations which I am about to recite also indicate the improbability of such influence in a large class of cases. The instances to which I call your attention are, I think, true reversions to ancestral types. Those of you who have observed the young non-blooming shoots of tulip-tree, sassafras and some other trees will have noticed that the leaves upon them often assume unusual shapes. Thus the leaves of sassafras often vary from the typical oval form to three-lobed and mitten-shaped upon the strong shoots. There are the most various forms on many tulip-trees, the leaves ranging from almost circular and merely emarginate to long-ovate and variously lobed; all of them have been most admirably illustrated and discussed recently by Holm in the proceedings of the National Museum. Holm considers the various forms of these liriodendron leaves to be so many proofs of the invalidity of the fossil species which very closely resemble them. This may be true, for there are probably no specific names of organisms founded upon so fragmentary and scant material as those applied to fossil plants; and yet I cannot help feeling that some of these contemporaneous variations are reversions to very old types. I was first led to this opinion by a study of the sports in ginkgo leaves, and finding them suggestive of Mesozoic types. "This variation in leaf characters," I wrote at the time, "recalls the geologic history of the ginkgo, for it appears to be true that leaves upon the young and vigorous shoots of trees are more like their ancestors than are the leaves upon old plants or less vigorous shoots, as if there is some such genealogical record in leaves as there is in the development of embryos in animals." Subsequent observation has strengthened my belief in the atavistic origin of many of these abnormal forms, and this explanation of them is exactly in line with the characters of reversions in animals and in cultivated plants. It would, of course, be futile to attempt any discussion of the merits of the specific types proposed by palæobotanists, but in those cases, like the ginkgo, where the geologic types are fairly well marked, constant and frequent, and where the similar contemporaneous variations are rare, there is apparently good reason for regarding contemporaneous forms as fitful recollections of an ancient state; and this supposition finds additional support in the ginkgo, because the species is becoming extinct, a fact which also applies to the tulip-tree, which is now much restricted in its distribution. I am further reinforced in this view by Ward's excellent study of the evolution of the plane-tree, for, in this instance, it seems to be well determined that the geologic type has fairly well marked specific characters, and the auricular or peltate base upon contemporaneous leaves, which records the connection between the two, is sufficiently rare to escape comment. Various writers have remarked upon the

similarities of these occasional leaves to geologic types, but, so far as I recall, they regard them as remnants or vestiges of the ancient types rather than as reversions to them. There is this important difference between a remnant and a reversion. A remnant or rudiment is more or less uniformly present under normal conditions, and it should give evidence of being slowly on the decline; whilst a reversion is a reappearance of wholly lost characters under unusual or local conditions. Now, my chief reasons for considering these sports to be reversions is the fact that they occur upon the sterile and verdurous shoots, the very shoots which are most likely to vary and to revert because they receive the greatest amount of food supply, as Darwin has shown to be the case with independent plants. And I am therefore able to make still another analogy between phytons and plants, and to illustrate again the essential sameness of bud-variations and seed-variations.

III.

I now wish to recall your attention more specifically to the subject of asexual variation. I have shown that no two branches are alike any more than are any two plants. I have also cited the frequent occurrence of differences so marked that they are called bud-varieties or sports. Carrière enumerated over 150 of them of commercial importance in France, and, as nearly as I can estimate, there are no fewer than 200 named horticultural varieties grown at the present moment in this country which had a like origin. It is also known that there are a number of species in which seeds are practically unknown, and yet which run into many varieties, as the pineapple, banana and bread-fruit; and note, if you will, the great variations in weeping willows, a tree which never fruits in this country. In our gardens there are three or four varieties of the common seedless 'top' onion, and I have been able, by treatment, to vary the root of the horse-radish, a plant which rarely, if ever, produces viable seeds in this climate; and there are variable seedless plants in our greenhouses. I might also cite the fact that most fungi are sexless, so far as we know, and yet they have varied into innumerable species. You will be interested in a concrete case of the apple. The Newtown Pippin, which originated upon Long Island, New York, has been widely disseminated by graftage. In Virginia it has varied into a form known as the Albemarle Pippin, and a New York apple exporter tells me that it is a poorer shipper than the Northern Newtown and is not so long-keeping. In the extreme Northwestern States the Newtown, while it has not been rechristened there, is markedly unlike the Eastern fruit, being much longer and bearing distinct ridges about the apex. Finally, in New South Wales, the ridges are more marked and other characters appear, and the variety is there known as the Five-crowned Pippin. This is not an isolated case. Most Northeastern varieties of apples tend to take on this elongated form in the Pacific Northwest, to become heavy-grained and coarse-striped in the Mississippi Valley and the Plains, and to take other characteristic forms in the higher lands of the South Atlantic States. This asexual variation is sometimes very rapid. An illustration came directly under my own observation (and upon which I have once reported) in the case of the Chilian strawberry. Within two years this plant, growing in my garden, varied or departed from its wild type so widely as to be indistinguishable from the common garden strawberry, which has been regarded by many botanists to be specifically distinct from the Chilian berry. This remarkable departure, which has enabled me, as I believe, to reconstruct the evolution of the garden strawberry, was one in

which no seedling plants were concerned. If all the common garden strawberries owe their origin to a like source—as I cannot doubt—then we have here a most instructive case of sexless evolution, but one in which the subsequent generations reproduce these characters of sexless origin by means of seeds.

This asexual modification is not confined to domesticated plants. Any plant which is widely distributed by man by means of cuttings or other vegetative parts may be expected to vary in the same manner, as much experiment shows; and if they behave in this way when disseminated by man they must undergo similar modification when similarly disseminated by nature herself. I need only cite a few instances of habitual asexual distribution of wild plants to recall to your attention the fact that such means of distribution is common in nature, and that in some cases the dispersion over wide areas is quite as rapid as by means of seeds; and some plants, as various potamogetons, ceratophyllums and other aquatics, are more productive of detachable winter buds and other separable vegetable organs than they are of seeds. The brittle willows drop their twigs when injured by storms of ice or wind, or by animals, and many of these cuttings take root in the moist soil, and they may be carried far down streams or distributed along lake shores; the may-apple and a host of rhizomatous plants march onward from the original starting point; the bryophyllum easily drops its thick leaves, each one of which may establish a new colony of plants; the leaves of the lake-cress (Nasturtium lacustre) float down the streams and develop a new plant while they travel; the house-leeks surround themselves with colonies of off-shoots, the black raspberry travels by looping stolons, and the strawberry by long runners; the tiger-lily scatters its bulb-like buds, and all bulbiferous plants spread quite as easily by their fleshy parts as by seeds. Now all these vegetative parts, when established as independent plants, produce flowers and good seeds, and these seeds often perpetuate the very characters which have originated in the asexual generations, as we have seen in the case of many bud-varieties; and it should also be remarked that these phytons usually transmit almost perfectly the characters acquired by the plant from which they sprung. Or, to put the whole matter in a convenient phrase, there may be, and is, a progressive evolution of plants without the aid of sex.

Now, where is Weismann's germ-plasm? One of the properties of this material—if an assumption can receive such designation—is its localization in the reproductive organs or parts. But the phyton has no reproductive parts; or, if it has them, they are developed after the phyton has lived a perfectly sexless life, and possibly after generations of such life, in which it and its progeny may either have remained comparatively stable or may have varied widely, as the circumstances may have determined. If any flower, therefore, contains germ-plasm it must have derived it out of the asexual or vegetative or soma-plasm. And I will ask where the germ-plasm is in ferns. These plants are fertilized in the prothallic stage, and one brief sexual state is all that the plant enjoys, after which the sex-organs die and wholly disappear. The fern, as the layman knows the plant, is wholly asexual, and the spores are as sexless as buds; yet these spores germinate and give rise to another brief prothallic or sexual stage, and if there is any germ-plasm at all in these fleeting sexual organs it must have come from the sexless spores. It is interesting to note, in this connection, this bud-variation is as frequent in ferns as in other plants. Or, if the Weismannians can locate the germ-plasm in all these instances, pray tell us where it is in the myriads of sexless fungi! There is no such thing as continu-

ous localization of germ-plasm in plants. Weismann himself admits that the germ-plasm must be distributed in 'minute fraction' in all 'somatic nuclei' of the begonia leaf, because that leaf is capable of giving rise to new plants, by means of cuttings, and all the plants may produce good flowers, which, if they are sexual at all, are so only by virtue of containing some of this elusive germ-plasm. There is no other way for these plants to get their germ-plasm, except from the somatic leaf from which they came. It would seem that this admission undermines the whole theory of the localization of the germ-plasm in plants, for one exception in the hypothesis must argue that there are others. But not so! There are no insurmountable difficulties before the Weismannians. It is the begonia which is the exception, for it is abnormal for plants to propagate by any such means! The answer which has been made to this statement is that very many plants are propagated asexually by horticulturists, and that all plants can probably be so propagated if there were any occasion for the effort. This answer is true; but the philosophical answer is that every phyton is an autonomy, and that the mere accident of its growing on the plant, in the soil, or in a bottle of water, is wholly aside from the point, for wherever it grows it lives at first a sexless life, it has an individuality, competes with its fellows, varies to suit its needs, and is capable, finally, of developing sex.

Another fundamental tenet of Weismannism is the continuity of the germ-plasm, the passing down from generation to generation of a part or direct offspring of the original germ-plasm. Now, if there is any continuity in plants, this ancestral germ-plasm must be inextricably diffused in the soma-plasm, as I have said, for every part or phyton of these plants, even to the roots and parts of the leaves, is able to produce sexual parts or germ-plasm. And if this germ-plasm is inextinguishably associated with every cell of the plant body, why does it not receive and transmit all incident impressions upon the plant? Why should acquired characters impress themselves upon the soma-plasm and not upon the germ-plasm when this latter element is contained in the very nuclei, as Weismann admits, of somatic cells? If the theory of the continuity of the germ-plasm is true for plants, then acquired characters *must* be transmitted! The only escape from this position is an arbitrary assumption that one plasm is impressionable and that the other is not; and, now, that we can no longer relegate the germ-plasm to imaginary deep-seated germ-cells, such an assumption is too bold, I think, to be suggested.

The entire Weismannian hypothesis is built upon the assumption that all permanent or progressive variation is the result of sexual union; but I have shown that there is much progressive variation in the vegetable kingdom which is purely asexual, and, for all we know, this type of modification may proceed indefinitely. There is no doubt of the facts; and the only answer to them which I can conceive the Weismannian to make is that these progressive variations arise because of the latent influence of ancestral sexual unions. In reply to this I should ask for proofs. Hosts of fungi have no sex. I am not convinced but that there may be strains or types of some species of filamentous algæ and other plants in which there has never been sexual union, even from the beginning. And I should bring, in rebuttal, also, the result of direct observation and experiment to show that given hereditable asexual variations are often the direct result of climate, soil or other impinging conditions. As a matter of fact, we know that acquired characters may be hereditary in plants; if the facts do not agree with the hypothesis, so much the worse for the hypothesis. Unfortunately,

the hypothesis is too apt to be capable of endless contractions and modifications to meet individual cases. I sometimes think that we are substituting for the philosophy of observation a philosophy of definitions.

I have, therefore, attempted to show:

1. That the plant is not a simple autonomy in the sense in which the animal is.

2. That its parts are virtually independent in respect to (a) propagation (equally either when detached or still persisting upon the parent plant), (b) struggle for existence amongst themselves, (c) variation, (d) transmission of their characters, either by means of seeds or buds.

3. That there is no essential difference between bud-varieties and seed-varieties, apart from the mere fact of their unlike derivation; and the causes of variation in the one case are the same as those in the other.

4. That all these parts are at first sexless, but finally may or may not develop sex.

5. That much of the evolution of the vegetable kingdom is accomplished by wholly sexless means.

There is, then, a fundamental unlikeness in the ultimate evolution of animals and plants. A plant, as we ordinarily know it, is a colony of potential individuals, each one of which, save the very first, is derived from an asexual parent, yet each one may, and usually does, develop sex. Each individual is capable also of receiving a distinct or peculiar influence of the environment and struggle for existence, and is capable, therefore, of independent permanent modification. It is not possible, therefore, that there is any localization or continuity of a germ-plasm in the sense in which these conceptions are applied to animals; nor is it possible for the plant as a whole to make a simple functional adaptation to environment. If there is a continuity of germ-plasm in plants this element must of necessity be intimately associated with every particle of the plant body, even to its very periphery, and it must directly receive external impressions; and this concept of Weismann—the continuity of the germ-plasm—becomes one of the readiest means of explaining the transmission of acquired characters. All these conclusions prove the unwisdom of endeavoring to account for the evolution of all the forms of life upon any single hypothesis; and they illustrate with great emphasis the complexity of even the fundamental forces in the progression of organic nature.

L. H. BAILEY.

CORNELL UNIVERSITY.

CURRENT NOTES ON PHYSIOGRAPHY (III.).

WOODWARD'S SMITHSONIAN GEOGRAPHICAL TABLES.

'THE average geographer,' to whose needs Professor Woodward has attempted to suit the recent volume of Geographical Tables issued by the Smithsonian Institution, should certainly feel highly complimented by this tribute to his quality. The volume contains, among many other matters, tables of coördinates for the projection of polyconic maps, lengths of a degree on parallels and meridians at different latitudes, areas of latitude-and-longitude, quadrilaterals of different dimensions and at different latitudes, adopted dimensions of the earth's spheroid, value of gravity at the earth's surface, and salient facts of physical geodesy. The latter heading includes the area of the earth, of oceans and continents, and the average heights of continents and depths of oceans, taken from Helmert's Geodäsie. For areas the continents are given 51,886,000, and the oceans 145,054,000 square miles. The mean depth of the oceans is placed at 3,440 meters. The mean heights of the continents are given as follows: The earlier results of Humboldt's, still often quoted, and the later ones of Penck (Morphologie der Erdoberfläche, 1894) being added for comparison.

	Humboldt.		Helmert.	Penck.	
Europe,	205		300	330 m.	
Asia,	351		500	1010	
Africa,	—		500	660	
Australia,	—		250	310	
America, North,	228 }	285	410	650 }	650
America, South,	346 }			650 }	
All Continents,	308		440	735	

The increase in the values of the latter measures is probably an approach to the truth, for early explorations frequently gave too much emphasis to narrow mountain ranges, and too little to broad plateaus.

A. AGASSIZ ON THE BAHAMAS.

A RECONNOISSANCE of the Bahamas and of the elevated reefs of Cuba, made by A. Agassiz in the winter of 1893, affords material for a Bulletin of 200 pages with 47 plates and many figures in the text, lately issued by the Museum of Comparative Zoology at Harvard College. The author is emphatic in rejecting the sufficiency of the Darwin-Dana theory of submergence in explaining the features of great limestone banks. The Bahamas consist of low hills of æolian limestone, "formed during a period of rest, during which the great beach of the then existing reef constantly supplied fresh material to be changed by the surf and the winds into sand for the heaping up of sand dunes. They could not be formed in a district of subsidence unless the subsidence was slower than the rate of growth of the corals, which is not the case in the Bahamas, as the reefs of to-day, even when they come to the surface, are not the sources from which the material for the great dunes of the Bahama Islands is derived" (p. 184, 185). At present the dunes are disappearing before the action of the sea. The conclusion of the reconnoissance seems to be that the great limestone banks are chiefly formed as 'marine limestones,' accumulating 'at great depths by accretion;' and that in the West Indies "wherever coral reefs occur, and of whatever shape, they form only a comparatively thin growth upon the underlying base" (p. 177). The text, with its figures, supplemented by maps and plates, gives an excellent idea of the geographical features of the region and of their evolution.

SPENCER'S RECONSTRUCTION OF THE ANTILLEAN CONTINENT.

PROF. MARCEL BERTRAND, of the École des Mines and the Geological Survey of France, has published an account of certain faint deformations of northwestern France, in which he interprets the inequalities in the floor of the English channel as the result of faint anticlinal and synclinal movements (Bull. Soc. Géol. France, xx., 1892, 118); thus implying that neither erosion nor deposition has been of significant measure in shaping the channel floor. Prof. J. W. Spencer takes almost the other extreme, and interprets certain inequalities of the ocean floor of the Antillean region, even to depths of twelve or fifteen thousand feet, as the results of river erosion during a not remote time when the entire region is supposed to have had a much greater altitude than at present (Bull. Geol. Soc. Amer., vi., 1895, 103–140); thus implying that no other processes than river erosion can account for the inequalities that he has traced. It must be concluded from these contrasted arguments that the forms of the sea floor are not yet so well understood as those of the land; because the facts are much less accurately known under than over sea level, because only form and not structure can be determined by soundings, and because the forms of the sea floor have received relatively little study. Where two specialists reach conclusions so unlike, it is difficult for others to choose between them; and for the present there will probably be some hesitation in adopting the teachings of the one or the other. With much interest aroused in the facts brought forward, and

with all willingness to look on the continents as unstable, it is difficult to believe that they have suffered changes so great as Spencer announces, not only in the uplift of the Antillean region, but in the deep depression of the axis of Central America, and in the denudation of the (inferred) great banks or continental shelf along the Windward Islands. The strongest proof will be demanded before vertical movements of two miles and a half can be accepted; and we fear that most readers will take refuge in a verdict of 'not proved.'

HISTORY OF THE ST. JOHN RIVER, NEW BRUNSWICK.

AN article on the 'Outlets of the St. John river,' by G. F. Matthew (Bull. Nat. Hist. Soc., New Brunswick, xii., 1894, 43–62), concludes that this river has been built up by contributions from three other systems, whose lower parts are now to be seen in the Restigouche, Miramichi and Petitcodiac. The evidence of this conclusion is derived from the geological structure of the country, beginning as far back as the Huronian time; the three rivers whose upper basins now belong to the St. John having been defined as basins of deposition in more or less remote geological periods. Thus the St. John river has attained its present magnitude by the breaking of mountain or hill barriers which once separated its three river systems, and is not a simple valley of continuous growth like the Mississippi (p. 55). The difficulty of accepting Dr. Matthews' conclusion as the only solution of the history of the St. John does not lie in any objection to the geological history of the region and its several basins of deposition, as far as stated, but in the omission of sufficient consideration of what has happened in the region since it became a land area. It has long been subject to subaërial erosion. During this time it has in all probability been variously warped and otherwise moved with respect to its base-level. Its rocks are of diverse resistance, and hence there may have been repeated opportunities for diversion and rearrangement of river courses during the long life of the region as a land area. While admitting that several geological basins of great antiquity are now drained by a single river, it does not necessarily follow that this river is an immediate descendant of the rivers which at one time or another drained the separate basins. The actual St. John river may once have been larger than now; its neighbors may have gained drainage area from it instead of losing drainage area to it; but these possibilities are not considered.

THE ORIGIN OF THE MISSISSIPPI.

THE reference to the Mississippi in the previous paragraph brings up an oft-encountered implication of simple history in the development of this great river, against which there is much evidence. A similar implication is found in a recent State Survey Report, where it is stated that, as a result of continental evolution at the close of the Carboniferous period, the drainage of the Ohio region was turned southward "into the great Mississippian bay, which then washed the shores of the new-born continent as far north as the mouth of the Ohio river" (Geol. Coastal plain of Alabama, 1894, 11). It is found again in the 'Story of the Mississippi-Missouri,' where the Mississippi at the close of the Appalachian revolution is described as heading somewhere in the Minnesota-Wisconsin region, and flowing southward to its mouth somewhere near the present city of St. Louis, whence a deep gulf extended southward to the present Gulf of Mexico (Amer. Geol. iii., 1889, 368). While the southward-flowing streams of the Wisconsin-Minnesota highlands are probably of ancient origin, the southward course of the Mississippi between Tennessee and Arkansas seems to

have been initiated not at the close of the Appalachian revolution, but long afterwards in Cretaceous time. The Appalachian revolution formed the mountains of Arkansas, as well as those of the Alleghany belt. The similarity of structure is so great that a trans-Mississippian extension of Appalachian growth may be reasonably assumed, as has been pointed out by Winslow (Bull. G. S. A., ii., 1891, 231). The existence of a bay, from the Gulf of Mexico northward towards St. Louis, is very improbable as a result of the Appalachian revolution; an east and west constructional mountain belt is a more likely product; and not until this mountain belt was well denuded to a peneplain did a later deformation depress it transversely, admitting the Cretaceous waters northward across it, and thus first forming the Mississippi embayment. Probably in part at the same time, and to a greater extent in later time, the denuded peneplains to the east and west were raised towards their present upland altitude, and as a result of this elevation the existing valleys and lowlands were opened in them during some part of Tertiary time. With these later elevations we may associate the uplift of the filled embayment and the southward growth of the Mississippi as a river. This view of the origin of the Mississippi embayment and of the date of the southward discharge of Mississippi drainage was first published by L. G. Westgate (Amer. Geol. xi., 1893, 251), as a result of conference with L. S. Griswold, who had then recently completed his investigation of the novaculite region of Arkansas.

THE CHUNNENUGGA RIDGE AND THE BLACK PRAIRIES OF ALABAMA.

IT is, perhaps, too much to expect that the origin of the physiographic features of a region should always receive due attention in a geological report along with the origin of its strata; yet there is no other place so appropriate for the official publication of physiographical discussions. It therefore occasions regret to find so little account of the origin and meaning of the Chunnenugga ridge and the Black prairies of Alabama in the elaborate report on the Geology of the Coastal Plain lately published by the Survey of that State. "The Chunnenugga ridge is made in great part by alterations of hard limestone ledges and bands of indurated sands of the Ripley. . . . . . It overlooks the low trough of the black prairies of the Rotten limestone towards the north with somewhat precipitous slopes in that direction, while its descent towards the south is much more gentle" (p. 356). It is manifest that the ridge with its inland-facing escarpment and the denuded inner lowland are typical features of a certain stage in the denudation of a coastal plain that consists of more and less resistant strata; the drainage of the lowland being chiefly gathered by subsequent streams that have been developed along the strike of the beds, and discharged by consequent streams which maintain transverse valleys through the enclosing ridge or upland. This general relation of form and drainage is so often repeated on coastal plains that its occurrence in Alabama deserves mention as a local example of a general physiographic feature; just as the Cretaceous strata on which it is developed deserve mention as local examples of a widespread geological formation.

W. M. DAVIS.

HARVARD UNIVERSITY.

THE NEW YORK MEETING OF THE ASSOCIATION OF AMERICAN ANATOMISTS.

THE Seventh Annual Session of the American Anatomists was held in the Medical Department of Columbia College, 437 West 59th Street, New York City, December 28 and 29, 1894.

The Association was called to order Friday, December 28th, by the President, Dr.

Thomas Dwight, in a few introductory remarks.

The report of the Secretary and Treasurer was read and accepted.

The Executive Committee recommended for election to membership the following names, and, on motion, the gentleman were elected:

1. Dr. F. J. Brockway, Assistant Demonstrator of Anatomy, Columbia College, New York City.

2. Dr. W. A. Brooks, Jr., Assistant in Anatomy, Harvard Medical School.

3. Dr. Franklin Dexter, Demonstrator of Anatomy, Harvard Medical School.

4. Dr. B. B. Gallaudet, Demonstrator of Anatomy, Medical Department of Columbia College, New York City.

5. Dr. R. H. Gregory, Jr., Demonstrator of Anatomy, St. Louis Medical College.

6. Dr. C. J. Herrick, Acting Professor of Biology, Denison University, Granville, Ohio.

7. Dr. P. C. Hunt, Assistant Demonstrator of Anatomy, Columbian Medical College, Washington, D. C.

8. Dr. Woods Hutchinson, Professor of Anatomy, Medical Department, University of Iowa.

9. Dr. W. P. Mathews, Demonstrator of Anatomy, Medical College of Virginia, Richmond.

10. Dr. Eugene A. Smith, Professor of Anatomy, Niagara University, Buffalo, N. Y.

11. Dr. P. Y. Tupper, Professor of Anatomy, St. Louis Medical College.

The Executive Committee, while not recommending affiliation with the Society of Naturalists, suggested that, as a rule, the Association should meet at the same time and place. This suggestion was discussed by Drs. Wilder, Spitzka, Dwight and Lamb, and was then adopted.

Dr. Wilder, from the Committee on Anatomical Nomenclature, reported progress. He also stated that Professor Stowell had resigned from the Committee.

The report of the Committee on Anatomical Material was called for. In the absence of the Chairman, Dr. Mears, Dr. Dwight reported progress.

The Committee on the Anatomical Peculiarities of the Negro also reported progress.

Dr. Huntington was elected to the vacancy on the Executive Committee, caused by the retirement of Dr. Spitzka.

The following papers were then read:

1. 'The best arrangement of topics in a two years' course of Anatomy in a medical school.' Dr. Gerrish. Discussed by Drs. Huntington, Baker, Wilder, Bevan, Allen, Shepherd, Lamb and Dwight.

2. 'History of the Development of Dentine.' Dr. Heitzmann.

3. 'On the Value of the Nasal and Orbital Indices in Anthropology.' Dr. Allen. Discussed by Drs. Wilder, Huntington and Dwight.

4. 'Loose characterizations of vertebrate groups in standard works.' Dr. Wilder. Discussed by Drs. Baker, Dwight and Allen.

5. 'The comparative anatomy of the cerebral circulation, with an exhibition of a series of anomalies of the circle of Willis.' Dr. Leidy. Read by title in the absence of the author.

6. 'Convolutions of the hemispheres of Elephas Indicus.' Dr. Huntington. Discussed by Drs. Wilder and Baker.

An inspection of the Medical Department of Columbia College was made in the evening, under the conduct of Dr. Huntington.

On Saturday, the 29th, the President appointed Dr. Gerrish to fill the vacancy upon the Committee on Anatomical Nomenclature, caused by the resignation of Professor Stowell.

The reading of papers was resumed:

7th paper. 'Classification of the tissues of the animal body.' Dr. Baker. Dis-

cussed by Drs. Heitzmann, Wilder, Dwight and Lamb.

8. 'Anomalies—Their significance.' Dr. Dwight.

9. 'Some muscular variations of the shoulder girdle and upper extremity, with especial reference to reversions in this region.' Dr. Huntington.

10. 'Some anomalies of the brain.' Dr. Wilder.

11. 'The correlation between specific diversity and individual variability, as illustrated by the eye muscle nerves of the Amphibia.' Professor C. Judson Herrick.

The discussion on papers 8 to 11, inclusive, was then opened by Dr. Baker, and continued by Dr. Shepherd (who illustrated his remarks with specimens), Dr. Wilder, Dr. Lamb (who also showed a specimen), Dr. Huntington, and concluded by Dr. Dwight.

Dr. Wilder exhibited a brainless frog and made remarks thereon.

On motion, the thanks of the Association were tendered to the College, and particularly to Dr. Huntington, for their hospitality.

The following members were present at some time during the session: Allen, Baker, Bevan, Bosher, Dwight, Ferris, Gerrish, Hamann, Heitzmann, C. J. Herrick, Huntington, Lamb, Moody, Shepherd, Spitzka, Weisse, Wilder. Total, 17.

CORRESPONDENCE.

A CARD CATALOGUE OF SCIENTIFIC LITERATURE.

EDITOR OF SCIENCE—*Dear Sir:* Your invitation to open in the columns of SCIENCE a discussion of the projected Catalogue of Scientific Literature to be prepared by international coöperation, the claims of which were presented in your issue of February 15, affords me a welcome opportunity to fall publicly into line with a great movement that I believe destined to prove of the highest importance to scholarship. As a few of your readers are aware, I printed privately, last summer, a brief circular advocating a similar enterprise. At the time of doing so I was at an out-of-the-way spot in the country, where it was impossible to exchange inspirations, except by post, with friends whose interest in the scheme might have been counted upon; but upon canvassing the subject in my own mind I became so convinced that the learned world was in sore straits in this matter, and that the way out was clear, that I felt sure I should presently discover that other restive spirits were beginning to agitate in the same direction. Little did I expect, however, to meet with so conspicuous and agreeable a confirmation of my premonition as came to me several weeks after the issuance of my circular (though dated before it), in the printed report of the Harvard committee, which has now appeared in SCIENCE. (The original communication of the Royal Society I have seen for the first time, through your editorial courtesy, in the proof sheets of SCIENCE.)

Although several of the suggestions contained in my own little circular were promptly outgrown by me, it may appear not inappropriate, on the principle of comparing small things with great, to reproduce here the contents of this highly aspiring but wholly unpretentious little document:

UNIFORM CARD MEMORANDUM INDEX.

The accompanying slip (size 2¼x3½ inches, 5.7 x 8.9 centimetres), designed to be cut out and filed alphabetically in the manner of a card catalogue, is printed as a tentative specimen of a projected **Uniform Card Memorandum Index**, and is herewith privately submitted to representatives of a few of the leading universities, learned societies and publication agencies, with a view to securing influential approval of the general plan, together with useful suggestions and criticisms as to its practical application. It is proposed that all the universities, learned societies and high-class periodicals of the world should coöperate (from January, 1895) in the production of such a uniform *memorandum index*, by publishing, as a supplement (or appendix, *or both*) to every number of their original publications, a brief slip-digest of the contents of each article—or even of important portions of each article, as may appear to be warranted. These supplements could be easily prepared (the digests being furnished in all or in most cases by the authors themselves), would be inexpensive both in their original form of publication and as separate slips, and would incalculably facilitate both the distribution and the classification for instant reference of all the newest results of discovery and research. Those interested in such a project are earnestly requested to communicate on the subject, before September 15, with the undersigned.

The specimen slip read as follows:

KINETO-PHONOGRAPH. PHONO-KINETOGRAPH. PHONO-KINETOSCOPE.
Edison, Thomas A., Invention of the Kineto-phonograph.
Century Magazine, June, '94, p. 206.
"In the year 1887 the idea occurred to me that it was possible to devise an instrument which should do for the eye what the phonograph does for the ear, and that, by a combination of the two, all motion and sound could be recorded and reproduced simultaneously. This idea, the germ of which came from the little toy called the Zoetrope, and the work of Muybridge, Marié and others, has now been accomplished, so that every change of facial expression can be recorded and reproduced life size. The Kinetoscope is only a small model illustrating the present stage of progress, but with each succeeding month new possibilities are brought into view, etc., etc."

The above circular, though sent to but comparatively few persons, called forth a gratifying number of 'adherences' and of valuable suggestions. In particular, the president of one of the American universities famous for activity in research and in the promulgation of knowledge undertook to have furnished, with the official *imprimatur*, summaries of the contents of all the publications of his university.

The necessity of entrusting the organization of the enterprise to a great central bureau that would command universal confidence early became manifest, and an informal communication on the subject was addressed to one of the officers of the Smithsonian Institution at Washington, who wrote in response: "I heartily favor the idea. When you have the matter in shape to make a formal proposition I shall have much pleasure in recommending it to the Secretary."

Meanwhile, from correspondence and conference with numerous scholars, various points involved in the success of the enterprise have grown in distinctness. The problem of utilizing more effectively the ever-increasing mass of accumulated, scattered and current contributions to knowledge can no longer be shirked. The time is ripe for instituting widely concerted action for recovering mastery of the situation. The various efforts hitherto directed to this end have done great service; but they have been devised almost exclusively to meet the requirements of reference and circulating libraries in their relations to broad classes of readers, rather than to serve the immediate needs of the individual scholar engaged upon a learned specialty.

All productive scholars, it would seem, must have devised or adopted for their personal use some form of *index rerum*, some mode—systematic or unsystematic—of note making. It is safe to say that very many such scholars have adopted for this purpose the general idea of the alphabetical card index, the merits of which are at present almost universally recognized. The scholar of Anglo-Saxon race is fast becoming as wedded to, and as dependent upon, his reference slips as the German scholar has long been silently devoted to his *Zettel* or the French savant to his *fiches*. It now remains for the Anglo-Saxon, with his openness to new applications of old ideas and the proverbial genius of his race for practical devices, to bring the power of the printing-press, as well as of scholarly coöperation, to bear upon the problem of multiplying indefinitely the benefits of the private card index.

Just here I should like to emphasize a consideration that is unexpressed, though latent, in the masterly report of the Harvard committee. This is, that such a card catalogue as is there projected, if based upon a wise choice in the *size of card* adopted, would render it possible for every member of the rapidly recruiting army of those employing the card system for private notes to incorporate his own manuscript or type-written cards and the printed cards (pertaining to his own specialty) of the coöperative index into one homogeneous whole, ever-growing, ever abreast of the latest research. This consideration it was, with all the possibilities and problems of administration it opens up, that held the mind of the writer under a spell of fascination for almost a week of vacation leisure. For be it noted that the blessings of the proposed coöperative card

index are to flow directly into the lap of the individual scholar, seated at his own desk in his private sanctum, enabling him to discard (not inappropriate word) to the limbo of the great libraries everything that does not directly concern him, while filing within reach of his finger-tips absolutely everything (pardon the optimism of an enthusiast) that he may intimately desire.

How can so Utopian a consummation be most speedily attained?

Let universities and colleges, and all manner of learned institutions and societies, at once appoint committees similar to the Harvard committee (though of course not limited to the natural and physical sciences, since the project of the Royal Society will form only a portion of the great undertaking), to accomplish three preliminary objects:

1. To arouse an intelligent and earnest interest in the subject.

2. To induce the Smithsonian Institution to assume the American leadership of the movement.

3. To convince publishers—primarily the publishers to the respective institutions concerned—of the importance of printing, on slips of the standard size, No. 33, of the American Library Bureau ($7\frac{1}{2}$x$12\frac{1}{2}$ cm., 3x5 in. approximately), summaries of their current publications for distribution as publishers' announcements. This size of slip is already widely in use, both publicly and privately, and may well prove to be of the dimensions ultimately adopted by the authorities of the projected international index. A beginning of these publishers' announcements has already been made by Messrs. D. C. Heath & Co., at the personal request of the present writer, and has been favorably submitted to the attention of the Secretaries of the Royal Society by Professor Bowditch, chairman of the Harvard committee. Other leading American publishers have heartily favored the idea of these card announcements and have promised to introduce them into use.

Columbia College has within a few days appointed, through its University Council, a committee to further the interests of the proposed International Coöperative Catalogue of Scientific Literature.

Yours very truly,

HENRY ALFRED TODD.

COLUMBIA COLLEGE, March 2, 1895.

PITHECANTHROPUS ERECTUS.

EDITOR OF SCIENCE—*Sir*:

In my letter of February 14th occur two expressions which need amendment. For the phrase 'divergent roots,' p. 240, 1st col., first line, read 'divergent root stems;' and for the phrase 'is wider than long,' p. 240, 2d col., fifth line, read 'is much wider than long.' Yours truly,

HARRISON ALLEN.

PHILADELPHIA, March 4th, 1895.

SCIENTIFIC LITERATURE.

Electrical Engineering, for Electric Light Artisans and Students. By W. SLINGO and A. BROOKER. New and revised edition, London, 1895. Longmans. Price, $3.50.

The object of this work is to cover general electrical engineering, and, taken as a whole, it is probably the most successful attempt yet made in this direction. The demand for a satisfactory general treatment of the applications of electricity is a very large and important one, and anything which supplies this demand is more than welcome. It is very doubtful whether any single work is ever likely to be published which will completely set forth the numerous and rapidly developing branches of electrical science and industry. Nothing short of an encyclopædia of many volumes could be expected to accomplish this result. A general discussion of the most important principles and uses of electricity, particu-

larly if it is not attempted to cover all branches, is a far more practicable problem, as the success of this volume demonstrates.

A work of this kind, however, is somewhat limited in its scope, since it is not intelligible to the ordinary untechnical reader, and is not of much use to the professional electrical engineer, who requires a more thorough and detailed study of each subject than is possible in a general treatise. This work would therefore be suited to one who had a certain amount of technical knowledge but who was not a specialist in electricity, for example, a mining or mechanical engineer, or a young man who had received a certain amount of electrical education at a technical or trade school and who wanted to learn more by his own efforts. It would also be useful as a textbook wherever a general course in electrical engineering is given. But in the opinion of the reviewer, a general treatment running from one subject to another is not the best way to educate electrical engineers of the highest type. This requires a careful and special study of each branch, aided by lectures and laboratory work, and the text-books should be entirely devoted to one subject, or, in fact, several books, each devoted to a small part of any one branch, is often preferable.

The authors of this book have had considerable experience as teachers and also the advantage of correcting and extending the contents of the first edition, which appeared in 1890, with the result that the new edition is well arranged and expressed and in most cases is brought reasonably well up to date. The first six chapters are devoted to general principles, units and methods of measurement. The next six chapters contain a treatment of dynamos and motors which is very satisfactory, considering the limitation of space. Transformers, secondary batteries, arc and incandescent lamps, are also well explained; but the last chapter, on 'Installation equipment, fittings, etc.,' is very meagre and the least satisfactory portion of the book. In fact, the principal criticisms would be that each element or device is explained as a separate thing, and no methods for combining these into systems are given. Nevertheless, it is a fact that the general design and arrangement of electrical apparatus is fully as important as the merits of each particular element. For example, the laying-out of a central station, or even a small isolated plant, determines its success or failure fully as much as the quality of the individual dynamos, lamps, or other particular parts of the plant.

The various systems for transmitting and distributing electric power, which is probably the most important branch of electrical engineering, are barely touched upon. In short, we may say that electrical engineering in its broadest sense is not covered, and probably was not intended to be covered, by this work. The subjects of electro-chemistry and electro-metallurgy, which now appear to be on the eve of important development, are not discussed. Telegraph and telephone apparatus and methods are not even mentioned.

These omissions, which are doubtless intentional and probably necessary, indicate that a complete treatise on electricity and its applications is almost an impossibility.

A few mistakes are noted; for example, on page 17, the International Ohm, adopted at the Chicago Electric Congress of 1893, is defined in terms of a column of mercury 106.3 centimetres in length and one square millimetre in cross section, whereas, the statement actually adopted was 'a column of mercury at the temperature of melting ice, 14.4521 grammes in mass, of a constant cross-sectional area and of the length of 106.3 centimetres.' This was intended to be exactly equivalent to a cross-section of one square mm., but it was put in this form because mass is more easily and ac-

curately determinable than cross-section. Another somewhat serious mistake, since it is fundamental, is the statement on page 18, that specific resistance is 'the resistance of any particular substance as compared with the resistance of a piece of some other conductor, such as silver, both being of unit dimensions.' As a matter of fact, specific resistance, which is a very important term, is the resistance in ohms of a unit volume, and is entirely independent of any particular standard substance. The use of the term 'magnetic resistance,' on pages 219 to 221, is open to objection, since the term 'reluctance' is now almost universally employed to distinguish this quantity from electrical resistance.

Taken as a whole, however, the errors are not numerous, and the work is recommended as a text or reference book for those who desire to learn the principles, general construction and action of the various kinds of electrical machinery and instruments, with the exceptions already noted. F. B. CROCKER.

PHYSIOLOGICAL PHYSICS.

On the Spontaneous Heating and Ignition of Hay. BERTHELOT. Ann. Chim. Phys., 7, 2. p. 430. 1894.

The author finds that poorly dried hay may ignite when the rise in temperature is only to 140° C. (280° Fh.). The evolution of heat necessary for this rise of temperature is due to the absorption of oxygen in spite of the interrupted sprouting, which will only take place when the hay is quite wet. The chemical process involving this absorption of oxygen may continue until the hay is thoroughly dry.

Druck und Arbeitsleistung durch Wachsende Pflanzen. W. PFEFFER. Abh. d. Math.-Phys. Kl. der K. Sachsicher Gesellschaft der Wiss., 20. p. 235. 1893.

Mr. Pfeffer investigated very carefully and ingeniously the pressure exerted by parts of plants in growth, and found, for example, that a root point could exert a pressure of 10–15 atmospheres. He ascribes these forces to osmotic pressure, and criticises the view concerning the growth of the cell-wall, which ascribes it to simple plastic expansion.

La Lumière Physiologique. R. DUBOIS Rev. gén. des Sciences, 5. p. 415 and p. 529. 1894.

Part first contains a review of light emitting organisms, and a description of the organs involved. In part second the author treats the subject of the emission more thoroughly, describing the character of the light radiated, and finds that the brightest Pyrophorus radiates 1, 4×10^{-7} calorie in ten minutes.

The author summarizes his extensive investigations as follows:

Neither a perfect organ nor a perfect cell is necessary for the coming and going of the light. The cell produces the photogenic substance which, once formed, may light or not, according to the conditions surrounding it.

They must fulfill the conditions necessary for life, must contain oxygen and water, and have a suitable temperature.

The light (luminous energy) is found to be 90% of the total energy radiated.

Dubois made a fluorescent substance from the blood of Pyrophorus, which, like that from the animal itself, lost its peculiar property on being treated with weak acetic acid and regained it on treatment with ammonia.

All the causes which excite or destroy the activity of the protoplasm have a similar effect upon the production of the physiological light.

The production of light depends upon the change of living protoplasmic granulations into the condition of lifeless crystalline matter.

It is to be remembered that the secretions of *Orya barbarica* are acid, thus in this case excluding the explanation of Radziszewski.

WILLIAM HALLOCK.

MATHEMATICS.

*The Principles of Differentiation in Space-Analysis.** By A. MACFARLANE, D. Sc., LL. D.

According to Hamilton the differentiation of a function of a quaternion presents novel difficulties due to the non-commutative character of a product of quaternions. There is in general no derived function, and it is necessary to define the differential in a new manner. Under certain conditions there is an analogue to Taylor's Theorem, but it is very complex, and no use is made of it. Hamilton does not differentiate the general transcendental functions, but only these functions restricted to a constant plane.

The author shows that these anomalies are true of products of vectors, but not of functions of versors. In versor analysis there is a derived function, satisfying a generalized form of Lagrange's definition ; and Taylor's Theorem takes on a form similar to that in ordinary analysis, only the order of the two quantities must be preserved. Let x and h denote two versors, then

$$f(x+h)=f(x)+f'(x)h+\tfrac{1}{2}f''(x)h^2+, \text{etc.},$$

provided the order of the x and h be preserved throughout.

The author finds the derived functions of various transcendental functions in space. He also shows that there are two essentially different meanings of $\sqrt{-1}$; one, when made definite, means a quadrant of rotation round a specified axis; while the other has no reference to direction, but distinguishes the area of a hyperbolic angle from the area of a circular angle. He also remarks that the theory of functions must be imperfect, because it is based upon a complex number which is restricted to one plane; no account is taken of the two essentially different meanings of $\sqrt{-1}$, and the idea of the versor is not distinguished from that of the vector.

* A paper read before the meeting of the American Mathematical Society, January 26, 1895. (Abstract.)

METEOROLOGY.

Neudrucke von Schriften und Karten ueber Meteorologie und Erdmagnetismus.

Dr. G. Hellmann, of Berlin, has undertaken the republication of certain old and rare writings relating to meteorology and terrestrial magnetism which have an important bearing on the history and development of these sciences. Very rare or typographically interesting works are printed in facsimile. Each reprint is preceded by an introduction, containing a general description of the book and its author. Although facsimile publications generally are so dear that only connoisseurs are able to buy them, yet, owing to the aid of the German Meteorological Society and its Berlin branch, the reprints are offered at a relatively low price by A. Asher & Co., Berlin. A few copies may also be had of A. L. Rotch, Blue Hill Observatory, Readville, Mass., at the publishers' prices. Each year one or two of the reprints will be issued, but the whole number will not exceed twelve. The following have already appeared :

No. 1. *Wetterbuechlein von wahrer Erkenntniss des Wetters.* REYNMAN, 1510. 41 pages introduction and 14 pages facsimile. Price 6 M. = $1.50.

This is the oldest printed meteorological work in the German language and was very popular, having 34 editions in seventeen years. Nevertheless, it is now so scarce that hardly thirty-six copies can be found.

No. 2. *Récit de la Grande Expérience de l'Equilibre des Liqueurs.* BLAISE PASCAL. Paris. 1648. 10 pages introduction and 20 pages facsimile. Price 3 M. = 75 cents.

This little work is of the greatest impor-

tance for the history of physics, meteorology and physical geography, since it furnishes proof of the existence of atmospheric pressure, and forms the basis of measurements of altitudes with the barometer. But three copies of the original are known to exist.

No. 3. *On the Modification of Clouds.* LUKE HOWARD. London. 1803. 9 pages introduction and 32 pages facsimile with three plates. Price 3 M. = 75 cents.

This was the first successful attempt at a cloud nomenclature on which all later schemes are based. The first edition of the original work is very rare.

A. L. ROTCH.

NOTES AND NEWS.

ENTOMOLOGY.

IT IS well to draw attention to two admirable brief illustrated papers published last year by Ch. Janet on *Myrmica rubra*, one on the morphology of the skeleton and especially of the postthoracic segments (Mém. Soc. Acad. de l' Oise, xv.), the other on the anatomy of the petiole (Mém. Soc. Zool. France, 1894). We regret we have not space for a full analysis of each, but they will be found of great interest to morphologists and hymenopterists. The clear illustrations are pretty sure to find their way into text-books.

The annual presidential address before the Entomological Society of London by Capt. H. J. Elwes is on the geographical distribution of butterflies and deals largely with those of North America.

Dr. Ph. Bertkau announces that his health obliges him to give up the admirable annual review of entomology which has appeared in the *Archiv für naturgeschichte* since 1838 under different editors—Erichson, Schaum, Gerstaecker, Brauer and Bertkau. Entomologists are under great obligations to Dr. Bertkau for the excellence of his summaries, their completeness and the promptness with which they have appeared. A still prompter method of rapid publication in all branches of biology is now being planned, which is at the same time a practical combination of all the current reviews —a consummation devoutly to be wished and helped forward.

M. Emile Blanchard was retired November last from the chair of entomology at the Jardin des Plantes, on account of age; his first entomological paper was published nearly seventy years ago; his successor has not yet been announced.

Fire has committed ravages with our entomologists this winter. Mr. J. G. Jack lost his library and collection in Jamaica Plain by the destruction of the building in which they were kept; Prof. C. H. Tyler Townsend lost his valuable dipterological library (nearly complete for America and very full for Europe) by the burning of the warehouse at Las Cruces, N. Mex., while he was absent for a few weeks at Washington; and now comes news that Rev. C. J. S. Bethune's school at Port Hope, Ont., has been burnt to the ground. His loss is estimated at eighty thousand dollars.

GENERAL.

AMONG the articles of scientific interest in the popular magazines for March are the following: Hermann von Helmholtz; Thos. C. Martin—*Century*. The World's Debt to Medicine; John S. Billings—*The Chautauquan*. Weather studies at Blue Hill; Raymond L. Bridgman—*New England Magazine*. Heredity; St. George Mivart—*Harper's Magazine*. The Direction of Education; N. S. Shaler—*Atlantic Monthly*.

PROFESSOR CARHART will deliver the address at the dedication of the Hale scientific building of the University of Colorado, on March 7th. His subject is *The Educational and Industrial Value of Science.*

THERE will be held at Vienna between the months of January and May, 1896, an

historical exhibition intended to bring under view the social and industrial condition of the country at the beginning of the century.

ARRANGEMENTS have been made that will probably ensure the union of the Astor Library, the Lenox Library and the Tilden Endowment. This would supply New York with a Library whose property is valued at $8,000,000.

A COMMITTEE of the English House of Commons has been appointed to consider changes in the system of weights and measures.

MR. CHARLES D. WALCOTT has been awarded the Bigsby Medal of the Geological Society of London.

LORD RAYLEIGH is delivering a course of six lectures on *Waves and Vibrations* at the Royal Institution of London. On April 5th he will lecture on 'Argon.'

THE Massachusetts Horticultural Society invites subscriptions for the erection of a monument in honor of the late Francis Parkman.

DR. KOSSELL, of Berlin, has accepted a call to the Professorship of Physiology at Marburg.

PROFESSOR C. L. DOOLITTLE, of Lehigh University, has been called to the chair of Mathematics in the University of Pennsylvania, and Mr. A. P. Brown has been appointed Assistant Professor of Geology and Mineralogy.

PROFESSOR JOHN B. CLARKE, of Amherst College, has accepted a call to a professorship of Political Economy in Columbia College.

DR. D. HACK TUKE, editor of the *Journal of Mental Science*, and well known for his writings on insanity, died in London, on March 5th, at the age of sixty-eight.

MR. J. W. HULKE, President of the Royal College of Surgeons of England, died recently at the age of sixty-five. He was eminent as a surgeon and especially as an ophthalmologist.

MR. HYMAN MONTAGUE, known for his writings on numismatics, died in London on the 18th of February, at the age of fifty-one.

PROFESSOR LAUTH, the eminent Egyptologist, died at Munich, on February 11th, at the age of seventy-three.

THE death is announced, at the age of eighty-five, of Sir Henry Rawlinson, the eminent Assyriologist.

MACMILLAN & CO. announce two works on Physical Geography, by Prof. Tarr, of Cornell University—one an elementary and the other an advanced text-book. The same publishers announce: *Louis Agassiz, his Life, Letters and Works*, by Jules Marcou.

SOCIETIES AND ACADEMIES.

BIOLOGICAL SOCIETY OF WASHINGTON, FEB. 23.

MR. F. E. L. BEAL read a paper on the food habits of woodpeckers, based on the examination of more than 600 stomachs. He found that the Hairy and Downy woodpeckers (*Dryobates villosus* and *pubescens*) feed chiefly on insects, most of which are harmful species. They also eat wild fruits and seeds. The food of the flicker (*Colaptes auratus*) consists largely of ants. Two stomachs contained each more than three thousand ants, and these insects formed 45 per cent. of all the stomach contents examined. The Flicker also ate other noxious insects and some wild fruit, such as dogwood berries and wild grapes. The Redheaded woodpecker (*Malanerpes erythrocephalus*) feeds largely on insects, all of which are harmful species except a few predacious beetles. The vegetable food of the Redhead comprises wild fruits and some corn and cultivated fruit. The Yellow Bellied woodpecker, or Sapsucker (*Sphyrapicus varius*), is

the only one in which the vegetable food exceeds the animal. It feeds largely on the inner bark and sap of trees, and also on insects. More than two-thirds of the latter in the stomachs examined were ants.

Dr. C. Hart Merriam, commenting on this paper, said that one result of the study of birds' stomachs by the Division of Ornithology and Mammalogy of the Department of Agriculture had been to show a wider range of food than previously suspected. Each bird has its favorite foods, but when these fail it is usually able to find something else on which it can subsist. Furthermore, the food of most species varies in different localities and at different times of the year, so that the examination of a series of stomachs, however large, from a single locality is utterly insufficient to furnish a reliable index to the range of food of the species. Thus, while the 600 stomachs of woodpeckers examined by Professor Beal failed to show a single beech-nut, it is nevertheless true that in northern New York beech-nuts form, during winters following 'nut years,' the principal article of food of three of the five species mentioned.

Mr. L. O. Howard remarked that it had been queried whether or not ants were more injurious than beneficial, and stated that as harborers of aphids and mealy-bugs they indirectly cause much damage, and are to be considered on the whole as decidedly injurious. He gave an interesting illustration of the manner in which ants had placed colonies of mealy-bugs on the artificially enlarged foliar nectar glands of certain Liberian coffee trees which had been placed in the hot-house of the Department of Agriculture.

Mr. F. A. Lucas described the general structure of the tongue of woodpeckers, noting the great difference between the tongue of the sapsucker (*Sphyrapicus*) and of most woodpeckers. In the sapsucker the tongue was of moderate length and margined for some distance back from the tip with hair-like bristles, some standing out, others directed backward, thus forming a brush for securing syrup. In the other woodpeckers examined, the tongue was excessively long and armed towards the tip with a few sharp, reverted barbs, an arrangement which seemed admirable for extracting grubs from holes in trees.

Mr. B. E. Fernow, in closing the discussion, said that he was glad to see the rehabilitation of the woodpecker, a bird which, once considered very beneficial, had been latterly condemned as injurious, while the evidence now presented seemed to be in its favor.

Mr. F. A. Lucas exhibited some Abnormal Feet of Mammals, saying that abnormalities in the way of digits could be mostly grouped under three heads, duplication of digits, irregular additions to the number of digits, the extra ones budding out from the others, and increased number of digits due to reversion. The latter he considered to be the rarest of the three, most of the extra digits of polydactyle horses being simply cases of duplication, as in the specimen shown. The feet of a pig exhibited illustrated the irregular addition of digits, while two feet of a three-toed cow were thought to be cases of reversion. Feet of an old and young llama illustrated the transmission of abnormalities.

Mr. M. B. Waite gave notes on the flora of Washington and vicinity, which were the result of his own collecting. Two species were added to the flora, namely: *Floerkia proserfinacoides*, Willd. (already published), and *Kyllingia primila*, Michx.

Selaginella rupestris, Spring, which had not been found for many years, was rediscovered at Great Falls. New localities were given for a number of rare plants. Attention was called to some spurious and doubtful additions to the local flora. The

tendency of some of the botanists to include in the flora cultivated plants or plants escaped from cultivation which do not properly belong there was criticised, as was also the practice of publishing plants in the lists of additions without seeing specimens and depositing them in some accessible collection. F. A. Lucas, *Secretary*.

NEW YORK ACADEMY OF SCIENCES, FEB. 11.
BIOLOGICAL SECTION.

The following papers were presented:

The Occurrence and Functions of Rhizobia. Dr. Albert Schneider. A discussion of the discovery of the adaptability of rhizobia to other plants than leguminous. Some conclusions based on investigations carried on at the Illinois experiment station were given to show that it is probable that rhizobia may be so modified as to grow in and upon roots of gramineous plants (ex. Indian corn).

An Undescribed Ranunculus from the Mountains of Virginia. Prof. N. L. Britton.

On the So-called Devil's Corkscrews of Nebraska. Dr. J. L. Wortman. A visit to the locality during the past summer had enabled him to study many problems in connection with their occurrence, which tend to throw considerable light upon their nature. The formation in which they occur was positively identified as the Loup Fork division of the upper Miocene, which is a true sedimentary deposit. The Diamonhelix occurs in a stratum of from 50 to 75 feet in thickness always standing vertically, and their tops are not confined to any one level. They vary much in size and character, but so far as observed always present the spinal twist. The fact that they occur in true sedimentary rocks, that their tops occupy many levels, together with the lack of evidence to show that there was any disturbance of level during the time the sediment was being laid down, was considered to totally disprove the theory that they represent the burrows of animals, which has been so extensively held in explanation of their curious nature. The invariable presence of plant cells, together with other facts, leads to the conclusion that they very probably represent the remains of roots or stems of some gigantic water plant.

The excretory System of Clepsine and Nephelis. Dr. Arnold Graf. The results of H. Bolsius have proved to be erroneous. The different parts of the nephridium are classified as follows: (1). *Infundibulum*, consisting in Nephelis of six bilobed ciliated cells, in *Clepsine* of a peduncle cell, pierced by a ciliated canal, and two bilobed ciliated cells attached to the peduncle. (2). *Receptaculum excretorium.* A vesicle which is in open communication with the funnel and in osmotic communication with the following parts of the nephridium. It is similar in both genera, and filled with disintegrating material. (3). *Portio afferentia.* The part of the gland, consisting of a single row of round cells, pierced by a sometimes bifurcated canal, which gives off branched canals. Similar in both genera. (4). *Portio glandulosa.* Row of cells, pierced by a smooth canal without side branches or bifurcation. This part is the largest part of the whole organ. Similar in both genera. (5). *Vesicula terminalis.* In *Nephelis* a vesicle, lined by a ciliated epithelium, in *Clepsine* a simple pouch of the *epidermis*, without cilia. (6). *Canalis terminalis.* The short canal by which the terminal vesicle communicates with the exterior. Present in *Nephelis.* In *Clepsine* it is equivalent to the terminal vesicle.

The cells formerly called *Chloragogencells* should now be called *Excretophores.* A preliminary account of these cells has been sent to the 'Zoologischer Anzeizer.' The investigation has been carried out mainly on living tissues, and every source of error has been eliminated.

Bashford Dean, *Rec. Sec'y.*

NATIONAL GEOGRAPHICAL SOCIETY.

CALENDAR, 1895.

Feb. 8.—*Topographic Forms:* MAJ. GILBERT THOMPSON, MR. HENRY GANNETT, MR. G. W. LITTLEHALES.

Feb. 15.—*Shakespeare's England:* REV. G. ARBUTHNOT.

Feb. 22. *Practical Results of the Bering Sea Arbitration:* MR. J. STANLEY-BROWN.

Mar. 1.—*Recent Discoveries in Assyria and Babylonia:* REV. DR. FRANCIS BROWN.

Mar. 8. *Mexican Boundary:* MR. A. T. MOSMAN, MR. STEHMAN FORNEY, CAPT. E. A. MEARNS, U. S. A.

Mar. 15.—*Turkey:* REV. DR. HENRY H. JESSUP.

Mar. 18.—*Washington to Pittsburg and to Niagara Falls; Across the Appalachians:* DR. DAVID T. DAY.

Side Trip to Niagara Falls: MR. G. K. GILBERT.

March 20. Reception at the Arlington Hotel, Washington, D. C., 9 to 11 P. M.

Mar. 22. *Pittsburg to Yellowstone National Park; Pittsburg to St. Paul, through the oil and gas regions:* PROFESSOR EDWARD ORTON.

St. Paul to Yellowstone National Park: MR. WALTER H. WEED.

Mar. 22.—*The Alaskan Boundary:* MR. J. E. McGRATH, MR. J. F. PRATT, MR. H. P. RITTER.

Mar. 25. *Yellowstone National Park to Sacramento; Yellowstone Park; down the Columbia; visit to Mt. Rainier and Portland:* MR. BAILEY WILLIS.

Portland to Crater Lake; Mount Shasta and Sacramento: MR. J. S. DILLER.

Mar. 29.—*Sacramento to northern Arizona; Sacramento; the Golden Gate; Yosemite; Los Angeles; San Bernardino:* MR. W. D. JOHNSON.

From San Bernardino across the deserts; to San Francisco Mt., Arizona: MAJ. J. W. POWELL.

Mar. 29.—*Oregon:* HON. J. H. MITCHELL.

April 1.—*Grand Cañon and Sonora, Mexico; Salt Lake City to the Grand Cañon; a winter in the depth of the Cañon:* MR. CHARLES D. WALCOTT.

Prescott, Phœnix and Tucson, to Sonora, Mexico; visit to the so-called cannibals: MR. W J McGEE.

April 5.—*Across the Rocky Mountains to Denver; Northern Arizona, the Rio Grande, and across the mountains to Denver:* PROF. A. H. THOMPSON.

The Home of the Pueblo Indians: MR. FRANK HAMILTON CUSHING.

April 5.—*Physical Geography of the Great Lakes:* PROF. MARK W. HARRINGTON.

April 8.—*Denver to Washington; Denver to Pueblo, down the Arkansas river, and across the plains to St. Louis:* MR. F. H. NEWELL.

St. Louis to Washington, with visits to the great caves of Ky. and Va.: MAJOR JED. HOTCHKISS.

April 12.—*Argentina,* Columbian University, 8:30 to 9:30 P. M.: DR. D. ESTANISLAO S. ZEBALLOS.

April 19.—*The Geography and Geology of Nicaragua:* MR. ROBERT T. HILL.

April 26.—*Antiquities and Aborigines of Peru:* MR. S. MATHEWSON SCOTT, MR. F. H. CUSHING.

May 3.—*Fredericksburg:* MR. W J McGEE, MAJ. GILBERT THOMPSON, GEN. JOHN GIBBON, U. S. A.

May 4.—Excursion and Field-Meeting, Fredericksburg, Va., 9 A. M. to 6 P. M.

May 10.—*President's Annual Address:* HON. GARDINER G. HUBBARD.

May 17.—Annual Meeting for the Election of Officers.

PHILOSOPHICAL SOCIETY OF WASHINGTON, MARCH 2.

On the Discovery of Marine Fossils in the Pampean Formation, by Dr. H. Von Ihering: MR. WM. H. DALL.

Classification of Clouds; Illustrated by lantern slides: MR. ALEXANDER McADIE.

The Army Magazine Rifle, Cal. 30: MR. ROGERS BIRNIE.

Additional Note on Gravity Determinations: MR. G. K. GILBERT.

WILLIAM C. WINLOCK, *Secretary.*

BOSTON SOCIETY OF NATURAL HISTORY, MARCH 6.

The Geographical History of the Lower Mississippi: MR. L. S. GRISWOLD.

Some Features of the Coastal Plain in the Mississippi Embayment: MR. C. F. MARBUT.

Note on cusped Sand-bars of the Carolina Coast: MR. CLEVELAND ABBE, JR.

SAMUEL HENSHAW, *Secretary.*

SCIENTIFIC JOURNALS.

THE AMERICAN JOURNAL OF SCIENCE, MARCH.

The Appalachian Type of Folding in the White Mountain Range of Inyo County, Cal.: C. D. WALCOTT.

Notes on the Southern Ice Limit in Eastern Pennsylvania: E. H. WILLIAMS.

The Succession of Fossil Faunas at Springfield, Missouri: S. WELLER.

Distribution of the Echinoderms of Northeastern America: A. E. VERRILL.

Drift Bowlders Between the Mohawk and Susquehanna Rivers: A. P. BRIGHAM.

Scientific Intelligence; Chemistry and Physics; Geology and Mineralogy; Botany; Miscellaneous; Obituary.

AMERICAN CHEMICAL JOURNAL, MARCH.

On the Cupriammonium Double Salts: THEODORE WILLIAM RICHARDS and ANDREW HENDERSON WHITRIDGE.

The Composition of Athenian Pottery: THEODORE WILLIAM RICHARDS.

A Redetermination of the Atomic Weight of Yttrium: HARRY C. JONES.

Separation of Nickel and Iron: E. D. CAMPBELL and W. H. ANDREWS.

Researches on the Complex Inorganic Acids: WOLCOTT GIBBS.

Cupric Hydride: EDWIN J. BARTLETT and WALTER H. MERRILL.

Action of Light on Lead Bromide: R. S. NORRIS.

The Action of Ammonia upon Dextrose: W. E. STONE.

The Carbohydrates of the Gum of Acacia Decurrens: W. E. STONE.

Reviews and Reports; Notes.

BULLETIN OF THE TORREY BOTANICAL CLUB, FEB.

New Species of Ustilagineæ and Uredineæ: F. B. ELLIS and B. M. EVERHART.

Contributions to American Bryology—IX: ELIZABETH G. BRITTON.

Japanese Characeæ—II: T. F. ALLEN.

Tradescantia Virginica var. *villosa* Watson: E. F. HILL.

Some new hybrid Oaks from the Southern States: JOHN K. SMALL.

Family Nomenclature: V. HAVARD.

Reviews.

Proceedings of the Club.

Index to Recent Literature Relating to American Botany.

NEW BOOKS.

Antisepsis and Antiseptics. CHARLES MILTON BUCHANAN. Newark, N. J., The Terhune Co. 1895. xvi+352.

A Laboratory Guide in General Chemistry. GEORGE WILLARD BENTON. Boston, D. C. Heath & Co. 1894. Pp. 163.

A Laboratory Manual in Organic Chemistry. W. R. ORNDORFF. Boston, D. C. Heath & Co. 1894. 82 experiments.

First Lessons in Chemistry. G. P. PHENIX. Boston, D. C. Heath & Co. 1894. Pp. 41.

The World of Matter a Guide to the Study of Chemistry and Mineralogy. HARLAN HOGUE BALLARD. Boston, D. C. Heath & Co. 1894. Pp. 264.

Physical Laboratory Manual. H. N. CHUTE. Boston, D. C. Heath & Co. 1894. Pp. vi + 213.

Practical Methods in Microscopy. CHARLES H. CLARK. Boston, D. C. Heath & Co. 1894. xiv + 219.